REPORT

TO

GENERAL SIR THOMAS MAKDOUGALL BRISBANE, BART.,

G.C.B., G.C.H., D.C.L., LL.D., F.R.S., F.R.A.S., H.M.R.I.A., PRESIDENT OF THE ROYAL SOCIETY OF EDINBURGH, AND CORRESPONDING MEMBER OF THE INSTITUTE OF FRANCE,

ON THE COMPLETION OF THE

PUBLICATION, IN THE TRANSACTIONS OF THE ROYAL SOCIETY OF EDINBURGH.

OF THE

OBSERVATIONS MADE IN HIS OBSERVATORY AT MAKERSTOUN.

BY JOHN ALLAN BROUN,

LATELY DIRECTOR OF THE OBSERVATORY.

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REPORT, &c.

To General Sir T. MAKDOUGALL BRISBANE, Bart.,
PRES. ROY. SOC. EDIN.

Sir,—I have several times brought under your notice the state of the Makerstoun Observatory, and of the Observations made in it, but generally with reference to some special object connected with the prosecution of the work itself; it seems desirable now, when the Observations have been reduced and printed, that I should report to you in a more complete way, in order to embody in the report some notice of the imperfections of the instruments and processes usually employed, and some suggestions for their improvement, the results of my experience in the management of your Observatory.

History.—The proposal made by Humboldt for obtaining an extensive series of corresponding observations in terrestrial magnetism, and generally in the physics of the globe, was readily prosecuted in different countries, but especially in the Russian and in the British Empires. In the latter, many observatories were founded,—by the British Government in the Colonies, and by the East India Company in the large territory under its sway. At home, our Government also founded an Observatory at Greenwich, under the powerful direction of the Astronomer Royal; in Dublin, another observatory was established at the expense of Trinity College, which was placed under the skilful hands of Dr Lloyd,—an observatory which was the Normal School for the Directors of the Colonial Observatories. Scotland alone, of the three kingdoms, seemed destined to remain without any share in this great scientific undertaking; a matter of double regret to the lovers of science in it, since an observatory in this country was even of more importance to the enterprise than in the more southern localities. What the British Government performed at Greenwich, and the rich institution of Trinity College did at Dublin, was undertaken by yourself in Roxburghshire.

The building of the observatory at your seat at Makerstoun was commenced early in 1841, but no observations were made till July of that year. The system adopted was limited, in the first instance, to a few daily observations, together with a participation in the complete series on *Term-days*. The observer, Mr Russell, had also the charge of a transit instrument and of several clocks. In

April 1842, Mr Russell having resigned, the observatory was placed under my direction, the same system of observation being continued. In the end of that year I suggested the propriety of increasing the number of daily observations, an increase which would require the services of another observer; this suggestion, which was seconded by the opinion of Professor J. D. Forbes, was at once approved by you, and Mr John Welsh, a talented student in arts of the Edinburgh University, was, at my recommendation, appointed by you as my assistant. The series of observations obtained in 1843 was necessarily still incomplete, no observations were made after 9^h P.M. nor before 5^h A.M., excepting on term-days and during magnetic disturbances: it was evident, therefore, that determinations of the diurnal laws would still be desiderata; while it was equally evident that a complete diurnal series would at once satisfy these wants, and render the previous incomplete series of greater value. I then suggested the addition of another observer to the establishment, pointing out, however, the most economical way in which this addition could be made, and especially the fact that it would render unnecessary the extra assistants required on term-days. These suggestions were made throughout with the full knowledge that they were in accordance with your object and wishes. I have merely to state the readiness with which you adopted this suggestion and every other tending to the advancement of the work of the observatory. Mr Alexander Hogg, an ingenious mechanic, who had been engaged in the construction of the observatory (which was performed chiefly under his superintendence), and who had been engaged under Mr Russell, at the first, and under myself afterwards, as an observer on the term-days, was recommended by me, as not only fitted for an observing assistant, but also as likely to be of much use in matters of handicraft connected with the observatory and its instruments; he was accordingly appointed in the end of 1843. In 1844 and 1845 a series of observations of all the magnetical and meteorological instruments was made hourly, excepting on Sundays.* It was originally proposed that this hourly series should extend through only two years; in 1846, therefore, the same system was adopted as in 1843. With the year 1846 terminated the period contemplated for the extended work of your observatory, and the period contemplated by the Royal Society of Edinburgh in adopting the large volumes of observations as portion of their Transactions. A more limited series was made in the following years, 1847, 1848, and 1849.

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* I may remark here that the omission of observations on Sundays, even of the great and unusual magnetic perturbations, which has excited the astonishment of our German co-labourers, and especially of the Baron Von Humboldt, like the omission of many other works on the same day, has never been felt as any loss. The amount of simultaneous disturbance observations in existence, made on the six week-days, is greater than we are likely to make any use of. It had not occurred, perhaps, to our continental friends that there was any difficulty in determining the kind of scientific work which would not be a "sin upon an English Sunday," as their own limit, if they have any, is not of a very stringent character.

The first part of the Makerstoun Observations was published in June 1845, and the last part appeared in June of the present year: the last part contains the detailed tables of *results* for the years 1845 and 1846, together with the general results of all the observations, including the monthly means of observations made in 1847, 1848, and 1849. The Makerstoun Observations have appeared as a portion of the Transactions of the Royal Society of Edinburgh, that body having liberally granted £600 towards the expense of printing; a grant for one object which, I am satisfied, will bear comparison with the grants of much richer societies.

I shall now notice some of the more salient matters in connection with the Observatory itself and its instruments.

The Observatory.—The geographical co-ordinates are—

Latitude, 55° 34′ 45″ N.
Longitude, $0^h\ 10^m\ 3^s{\cdot}5$ W. of Greenwich.

Height of the barometer cistern above mean water at Berwick, 213 feet.

The observatory is constructed of wood, like that at Greenwich; its architectural character is not of the highest order; it is placed on a small rising ground which is probably formed of felspathic trap, covered with rolled boulders, pebbles, and gravel. The internal division of the building, and the number, size, and positions of the windows are ill adapted for the purposes of the observatory. The plan and elevation were due to an assistant in the Dublin Observatory. Care was taken to exclude iron completely from the structure, copper nails having been used throughout: the telescopes and instruments were placed upon excellent white sandstone pillars, well-founded, and unconnected with the floor. The care taken in this matter is the most deserving of praise in connection with the construction of the observatory, and that care was chiefly or altogether your own. It should not be forgotten that the knowledge of the best arrangement for such a building was very small at the time it was planned, if it is considerable now.

The range of temperature within the observatory was much too great, owing to the thinness of the walls, and the number and largeness of the windows. The building was at first heated by copper stoves which soon became oxidated, and produced suffocating fumes of most hurtful character. It was altogether impossible to approximate to anything like uniformity of temperature in a house so easily affected by external variations. The stove was discontinued in 1843, and a small brick building erected at a distance for a computing room in winter. I proposed, in 1843, to heat the observatory with hot water pipes, but the difficulties connected with this process were too considerable to render the experiment desirable. It was found possible in 1845, to erect a fire-place with a tubular brick chimney in one of the ante-rooms, which, though wholly useless for heating the observatory, served to render the small room in which it was placed sufficiently comfortable for a computing-room.

The Instruments.—The declination and bifilar magnetometers were made by Mr Grubb of Dublin. The magnets during 1841 and 1842 were enclosed by loosely fitting cylinders, resting upon the pillar tops, covered by glazed lids; but the junctions were neither pressed together by any contrivance, nor was there any moderate precaution taken to prevent currents of air from circulating in the boxes. In 1843 and 1844, these and various other defects were removed: the joints were lined with velvet and pressed firmly together; rectangular boxes were placed within the drums; both cylindrical and rectangular boxes were gilt outside and inside; and the whole apparatuses were covered by thick cotton hoods. The gilding prevented the effects of radiation in producing currents within the boxes; the other precautions prevented the entrance of external currents, and secured to a considerable extent a uniform temperature and dryness.

The suspension threads for the declination magnet broke at short intervals in 1841 and 1842; they were succeeded in June 1843 by a thread carefully prepared, which is still in use, and apparently as good as ever. In case, however, the suspension thread should have received any torsion, the magnet was removed occasionally, a brass bar was suspended, and the torsion was rendered zero for the magnetic meridian before replacing the magnet; when little torsion was found existing the trials were made seldomer, as the mere manipulation was apt to introduce torsion of itself.

Errors due to twist in the suspension wires of the bifilar magnet, were eliminated by the process employed to determine the unit-coefficient. The interval of the wires was adjusted according to the instructions by the Committee of the Royal Society; but this rendered the instrument too sensitive, and, during moderate disturbances, the scale went out of the field of the telescope. This was attempted to be remedied at the time by turning the torsion circle till the scale reappeared, generally a difficult and unsatisfactory operation; in 1846, however, I employed a small magnet for the purpose, this being placed at known distances where its effect upon the bifilar magnet was determined.

Even for the ordinary diurnal movements, however, the instrument was too sensitive; the reading of the magnet scale was estimated to one-tenth of a division, and of the thermometer to one-tenth of a degree, but a variation of one-tenth of a degree in the temperature of the magnet is equivalent to two-tenths of a scale division in its motion. The thermometer was placed with its bulb resting on a brass bar, so that it might show as nearly as possible the temperature of the magnet.

The balance magnetometer was made by the late Mr Robinson of London; the box was gilt inside and outside, and covered with cloth as in the case of the other instruments. The magnet was never lifted from its support, excepting during the adjustments; it was vibrated through small arcs only. Many of the apparent anomalies which I have observed in this instrument, and described in the Edinburgh Transactions, were probably due to the facts, that the needle rests upon knife edges, and that every care being taken in the management of the instrument, the consequences of such a mode of support were exhibited under the most favourable circumstances.

I have referred to the imperfections of the inclinometer, in the introductions to the various volumes; one error I believe to be due to the presence of iron in the copper circle or basement of the instrument.

The barometer, a standard by Mr Newman, was fixed to the south wall of the west ante-room, not the best possible position in the observatory, as it was subjected to greater variations of temperature than it would have been if placed to the north. The thermometers were placed on a revolving frame in front of one of the north windows; but the case was removed to the west in the morning, and to the east in the evening, when the sun shone near to, or north of the prime vertical. The anemometer is sufficiently well-fitted for the determination of the laws of variation, but there is considerable uncertainty as to its value for giving absolute results, as there is with most of the anemometers yet in use. From various causes, the working of the anemometer became less trustworthy in 1848 and 1849.

Times of Observation.—The ordinary observations were made at certain previously arranged hours and minutes of Göttingen mean time. When the observer, from any cause, was too late for his observations, he was requested to note the exact minutes when the observations were made; this was done in all cases, and the true minutes will be found in connection with the printed observations. No check-clock was used, and none was necessary. Indeed, it is my opinion, where little confidence is placed in some portions of the labours of assistants, they will generally, in consequence, be found to deserve little trust in others; and this is especially true in the work of an observatory, requiring such a *various* fidelity and care. Besides the regular observations, large masses of observations were made during magnetic disturbances and other occasional phenomena.

Reductions.—Before the reductions of the magnetical observations could be completed, certain factors were requisite, representing the effect of one degree Fahrenheit on the positions of the bifilar and balance magnets, and the values of a given change of angular position in terms of some known or easily found unit. It is not necessary that I should do more than refer to the fact, that I have proved the insufficiency of the processes originally prescribed for the accurate determinations of those co-efficients and have substituted new ones, of the accuracy of which there can be no doubt, as they have been verified in every possible way.* I mention this, chiefly to remark upon the time and labour bestowed on the investigations for these important objects. Could I have been satisfied with issuing the volumes of observations from the press uncorrected, as has been done in many cases, or have reduced them by the official methods, the volumes might have been published immediately after the observations were made, as all the ordinary portion of the reductions were performed, and were verified week by week. The whole value of such observations, for any present use, depends upon these reductions: erroneously corrected observations are as useless as if they were uncorrected; and to publish them in an uncorrected state would be simply to place the volumes on the shelf, with large masses of other like valuable works, till their proper reduction and discussion at the period vulgarly denominated the Greek Calends. It is conceived, therefore, that the great amount of labour was well spent, which was necessary, *first*, in attempting to obtain satisfactory results from the old processes; *second*, in gradually developing and perfecting the new ones; and, *third*, in rendering the factors deduced as accurate as possible, by getting rid of those errors which are unavoidable in the employment of new methods. It is conceived also, though this well-bestowed labour were forgotten, and by far the greater part of it has never shown itself in print, that the speed with which the volumes have appeared, will do no discredit to your observatory, nor to its director with one complete and one partial computing assistant.

Printing.—Considerable time was bestowed in arranging the observations into the best forms for printing; many condensations and improvements were gradually adopted till, it is believed, that the volumes for 1844, 1845, and 1846 have been printed in the most distinct and condensed way to be unabbreviated; it would have been a great saving of *time* to have had them printed in twice the bulk. The proofs were carefully revised by my assistants and myself; almost every sheet passed through my own hands before sending to press.

Division of Labour.—The general management of the observatory, with the correspondence and all other matters relating thereto, devolved upon myself. I took a regular share of the work of observing night and day, especially in the years 1843, 1844, and 1845, and, with Mr WELSH, performed the principal portion of the work of computing, and of the more difficult class of absolute observations. I also made, with Mr WELSH, the astronomical observations of transits, &c., and performed other occasional work required by yourself.

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* The imperfect system of determining the temperature coefficients by removing the magnet from its place in the instrument to water baths, has been retained in opposition to anything like scientific caution, after its insufficiency has been demonstrated in the case of instruments by the best makers, treated with *at least* an average care.

Mr Welsh shared in all the work of observing and computing. A large mass of observations of the new planets were made with your large equatorial, chiefly by Mr Welsh, at extra-official hours; these observations have been published by the Royal Astronomical Society.

Mr Hogg shared in the regular and disturbance observations, assisted in the computations, and performed all the mechanical work connected with the observatory and its instruments.

I have already noticed, in different Introductions to the Makerstoun Observations, the diligence and care of my assistants. I shall here repeat my obligations to Mr Welsh, who was trained from the commencement under my own eye. His acquaintance with the mathematical details of his work, his skill as an observer, his fidelity in all he performed, and his appreciation of the practical difficulties occurring, have been of the greatest importance to me. He was made conversant with all my own views upon the theoretical and practical questions which so often developed themselves, and, in return, I frequently derived considerable benefit from his opinions.

To Mr Hogg, also, I can award my testimony as to the honesty and diligence with which he has fulfilled the duties for which he was at first engaged. To his invention and handicraft most of the mechanical contrivances, in wood and brass, about the observatory are indebted.

Suggestions.—I shall now offer a few suggestions, the results of my experience, on the subject of magnetical and meteorological observatories, especially in such climates as our own.

Situation.—I need scarcely say that it is of importance, especially for absolute magnetical determinations, that the position chosen for the observatory should not be over highly ferruginous rocks, even though observations can be made in the neighbourhood to determine any local error. But there is a point connected with the placing of magnetical and meteorological observatories which it seems to me is of very great importance, a point which has been apparently overlooked to a very great extent; I mean the influences due to the proximity of large cities. These influences, I fear, are much greater than may be suspected, and that not only on the absolute values, but likewise on the variations, whether magnetical or meteorological. Suppose an observatory placed on one side of a large city, which contains immense masses of iron, huge piles of stones, and thousands of human beings; this combined mass has a temperature several degrees higher than that of the air at a distance; it is a generator of electrical, thermal, and aërial currents: have these no effects upon the magnetical and meteorological states of the neighbouring district? The probabilities appear all in favour of an answer in the affirmative, and, at least, till the negative can be proved, it is evidently contrary to all sound investigation to incur the risk of ills we know not of. I feel inclined to attribute some curious discrepancies betwixt the results obtained in town and country observatories to some such causes.

The building to contain the variation instruments and computing room, should be made of stone if possible, even though it should contain some *small* quantity of iron. It is of the greatest importance that the temperature within the instrument room should be nearly uniform; for this end, stones or *logs* of wood are essential; I do not believe that any small quantity of iron which analysis might detect would affect the *variation* results. In the construction of the observatory, however, an important matter has always been lost sight of: it is nearly impossible that the temperature in our climate can be very uniform without a complete exclusion of the external atmosphere. Such an exclusion has been attempted by sealing up the observatory, and by burying it under ground; methods which might have served for the instruments, but which would in a short period have served for the observers too. It appears essential, then, that the instruments should be placed in a different room from the observer: this room should be wholly *within* the observatory, with passages or rooms around it, separating it on all sides from the outer wall: the telescopes could be placed in the wall of this room, the eyepieces being outside; a single glazed aperture with a lamp and proper arrangement of mirrors would serve to illuminate all the scales. There should be no windows, and only one (*double*) door into the room which could be well closed, and would require to be opened only at considerable intervals. The roof of the observatory should slope only to the north, so that the sun could not beat on it, or some arrangement should be made by a double roof, to prevent the heating there from affecting the internal temperature.

By such arrangements, the diurnal variation of temperature would be scarcely appreciable within the magnetometer boxes. It would be necessary, in order to determine the temperature co-efficients, either that this room could be opened for some time to the variations of the external temperature, or that it should be possible to heat it artificially: this might be done by a stove, or by pipes with hot water laid in the external room, which could be used as a computing room. The annual variation of temperature might be very much diminished, and the dryness of the instruments be insured by this arrangement. It is to the effects of varying temperature and humidity, that the principal errors are due for the three variation magnetometers.

Having suggested what I conceive to be the best position for the variation magnetometers, I would remark, shortly, with reference to the dry and wet bulb thermometers, that they ought not to be placed in a *recess* to the north; a position which, although sufficiently shaded from the sun, is, however, in general wholly abnormal; it is generally damp, and if the house be of stone, of a considerably lower temperature than in the shade elsewhere.* The thermometer case should, I think, be placed to the north of the building, not in a recess, but sheltered from the sun's rays in the morning and evening by large double boards placd at a moderate distance, and permitting a free circulation of the air: the case itself should be similar to that employed at Makerstoun, or of a lighter construction, which can be turned round from within the observatory, so as to allow the observations to be made from one of the windows, the glass of which prevents the effects of radiation from the body of the observer, or from the lamp during night.

The Declinometer.—This instrument, which is of the simplest possible construction, requires no correction for the effects of temperature; it has generally, therefore, been *the* instrument in observatories, the only one from which laws of variation might be expected with little trouble, yet the observations have in general been overrun with errors. The errors of the instrument are due chiefly or altogether to the suspension of the magnet.

The French Academy of Sciences thought the mode of suspension of the declination-magnet of so much importance, that they offered it as the subject of a prize in the year 1777; previously the needle was balanced upon a pivot as in our common mariner's compass. The prize was carried by COULOMB, who proposed suspension by means of a thread formed of the silk fibres from the cocoon. This suspension was adopted immediately after by M. DOMINIC CASSINI; the old cap and pivot suspension was used, however, for some time afterwards, as by GILPIN, early in the present century. CASSINI seems to have soon become aware of the effects of humidity, and of separation of the fibres, in introducing torsion into the thread; for he prepared his threads by first gumming the fibres together, and then greasing the thread. It is obvious, however, that, after all, the threads thus constructed must have performed very indifferently; for his observations, which were made at the same time in the Paris Observatory, and in the caves below, shew differences and variations in the monthly means which are not explicable by anything within our later, and, as we may suppose, more accurate experience. Since CASSINI's time, the improvement of the suspension-thread seems to have made very little progress. M. KUPFFER, apparently despairing of satisfactory results from a silk suspension, substituted silver wires in the Russian declinometers; a similar suspension has also been employed by M. QUETELET at Brussels. This seems to me a step towards the cap and pivot suspension. Indeed, M. NERVANDER of Helsingfors has found that such suspension cannot be trusted, since the wires are so affected by temperature that, when an unmagnetic bar is suspended, it has a considerable diurnal motion: a fact which I had suspected, and had pointed out as a probable source of error in determining the temperature coefficient for the bifilar magnet.

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* As I will not again allude to the meteorological instruments, I may mention here my doubts as to the accuracy of the theory of the wet bulb, even as a measurer of the humidity of the locality in which it is placed; and my perfect belief that the determinations of the vapour pressure in the atmosphere, obtained by means of the dry and wet bulbs, are wholly in error. I have indicated this opinion in different volumes of the Makerstoun Observations, where I have also shewn that the apparent success of an attempt made by M. DOVE, and lately by Colonel SABINE, to resolve the diurnal variation of the total pressure of the atmosphere into two simple variations, is in all probability due to the large but erroneous diurnal variation of aqueous vapour pressure obtained in the mean for the year; which, when subducted from the smaller double variation of the total pressure, leaves traces only of its own abstraction. I have shewn that this is the more certainly true, since, when we consider winter only, the variation of the aqueous vapour pressure, as computed, being then very small, the pressure of the *dry air* then exhibits a well marked *double* diurnal variation as before. I do not enter in this Report into the Results of the Makerstoun Observations. I do not mention the previous fact to shew that the pressure of aqueous vapour is not involved in the diurnal variation obtained from the barometer, but as some evidence that the pressures deduced from the dry and wet bulbs are not to be trusted. I have proposed and attempted some experiments for the purpose of obtaining the *actual* value of the vapour pressure in the atmosphere, by destroying the moisture in a closed apparatus, but have not yet succeeded, owing to imperfections in the instrument.

I am glad to learn, since the above was written, that Colonel SYKES has also, in a paper read before the Royal Society of London last session, objected to the results from the psychrometer.

The suspension-thread acts in the following manner:—As the thread is composed of a series of fibres more or less twisted, the plane of detorsion, that is, the vertical plane in which an unmagnetic bar will rest when suspended, is determined by the composition of series of opposing forces: if the torsion of the individual fibres be at all considerable, very small motions of the magnet will cause them to occupy slightly different positions *inter se*, or moderate changes of humidity acting to a greater extent upon the external than the internal fibres, and upon some of the external fibres than upon others, will change the plane of equilibrium, and in this way force the magnet from its true position. Changes of the plane of detorsion caused in this way, and by the occasional breaking of fibres, will explain the great discrepancies occurring in large series of observations, and the consequent lessening of their value. The importance also of obtaining properly-constructed suspension-threads will be at once evident, when it is remembered, that unless such are obtained, the labour of years may be rendered of little or no value. The conditions sought to be obtained in the construction of the thread for the Makerstoun declinometer, were the following:—*First*, The absence of all torsion from the fibre. The so-called *untwisted* compound fibre from the cocoon, usually employed in observatories, receives a considerable twist in the operations of drawing from the cocoon and reeling, as may easily be perceived by passing it between the thumb-nail and index, the method which I employed to remove the twist. *Second*, That each fibre when combined into the thread, shall bear an equal portion of the weight. For this end the fibre was not cut into pieces, but a sufficient length being obtained, free of flaws, it was wound round two smooth pins, placed at the required distance, so that no twist should be introduced in the act of winding; when a sufficient thickness was obtained, and the ends were tied, a hook attached to a weight was inserted in place of the lower pin; the thread being formed of one continuous fibre was thus free to move round the upper pin and the weight-hook till each length of fibre bore nearly an equal strain. *Third*, That the thread should be as small as is consistent with durability. The number of fibres for a particular weight should be determined by experiment; sixteen fibres of the silk supplied to the Makerstoun Observatory, and bearing nearly a pound weight, were found insufficient several times, though that number was recommended in the Report of the Royal Society; this was probably due to some difference in the thread: a thread of 22 fibres has now performed well for seven years.

I should notice that M. Nervander has proposed to form the suspension thread, by moistening with hot water the fibre cut into lengths, and submitting each length in this state to a considerable tension, before combining them to form the thread.

The Bifilar Magnetometer.—The chief source of error for this instrument is also to be found in its suspension; wires of silver or of gold have generally been adopted; although threads like those for the declinometer have also been employed, as at Greenwich. I object to the use of skeins, not merely because the errors due to a silk suspension are probably greater than those due to wires, but chiefly because no correction can be applied for the errors due to the former, while those due to the latter can be wholly eliminated. One error common to every kind of suspension is due to the stretching of the thread or wires, which gives a false value of the secular change; it is curious that no attempt has been made to eliminate this error, which could easily be done by means of a small apparatus for measuring the distance between the suspended magnet and the base plate of instrument. Any twist in the wires will give a false value of the unit coefficient, if determined in the usual way by the torsion circle; I am afraid that there is an equal chance of a similar error with silk threads, besides the probable variation due to breaking of fibres, &c. The error arising from twist, however, does not appear, when the process, which I have proposed and used, of determining the unit coefficient by deflections is adopted. The error which is peculiar to the wire suspension is that already noticed of a variation due to temperature; this error, however, also disappears when the temperature coefficient is determined by my process, since then, the total effect of temperature upon the position of the magnet is at once obtained. There remains against the silk suspension the heavy and indeterminable errors due to humidity, and to the gradual breaking of fibres.

By the use of metallic wires and an apparatus to determine the amount which they stretch, nothing is required to render this instrument as perfect as possible but a magnet with permanent magnetism, the proper processes being employed for the determinations of the unit and temperature coefficients.

The Balance Magnetometer.—I conceive that with proper care this apparatus may give considerably better results than have ever yet been obtained from it.

The points of principal consequence are, well made hardened knife edges, and a magnet that will not lose magnetism. The temperature coefficients of well hardened thin bars, especially of those supplied by Robinson, have been very small; and it would not be difficult for the maker, by a few experiments, to diminish or destroy the effect of temperature altogether in the manner suggested afterwards. The only subject then remaining for the maker's skill would be the formation of a powerful and *permanent* magnet with a hard well-made knife-edge. I have already shown in other places, than no dependence can be placed on the coefficients involving the time of vibration of this needle.

Mechanical Temperature Compensation.—The difficulties connected with the diminution of the temperature coefficients of magnets, for the purposes of the bifilar and balance magnetometers, may be, to a great extent, avoided by mechanical compensations. Such compensations, by a little experimentation and previous calculation, may be sufficiently complete for most self-registering apparatuses, to render considerations of the varying temperature negligible: even for the more delicate apparatuses, however, any incompleteness of the temperature compensation may be determined by the process which I have employed. The following method of compensation may be adopted for the bifilar magnet:—Let the upper extremities of the suspension wires be attached to the ends of two brass rods, which approach each other within an interval equal to the diameter of the lower wheel, and let the other ends of the brass rods be *fixed* to a beam of wood, so that an increase of temperature will cause the free ends of the rods to approach each other, by an amount equal to the difference of their expansion and that of the wooden beam to which they are fixed. Such an approximation will diminish the directive force of the wires, and by a proper regulation will compensate for the diminution of the magnet's moment. If the suspended wires are silver, and the lower wheel is of brass, the coefficient (e) of *contraction* of the space betwixt the suspension wires at the top will be found from the equation of equilibrium to be

$$e=q$$

where q is the temperature coefficient. Thus, in the case of the Makerstoun bifilar $q=0{\cdot}000266$, the interval of the wires is nearly 0·5 inch, and, therefore, the brass rods would require each to be about $7\frac{1}{2}$ inches long, in order that the interval be diminished 0·000266 of itself, or 0·000133 inch; the difference of the coefficients of expansion of brass and wood being assumed $=0{\cdot}0000085$. Magnets with a temperature co-efficient of 0·0001 would require brass suspension rods of3 inches in length, or less in proportion as the interval of the wires is less than 0·5 inch.

For the balance magnet; let a brass rod be fixed to the magnet near its south end, but free to expand towards the north, and having as much to the north of the axle as to the south; it is obvious that when the temperature increases the expansion of the brass rod towards the north, if properly regulated, may be made to depress the north end of the needle as much as the diminution of the magnetic moment would tend to elevate it. The expression for the proper weight and length of such a brass rod is more complicated than in the case of the bifilar, depending on the weight of the needle, and the distance of its centre of gravity from the centre of motion. In the case of the Makerstoun balance, for which $q=0\cdot00008$, I have computed that a brass rod 10 inches long, $\frac{1}{30}$ the weight of the needle, fixed so that its centre of gravity should be near the centre of motion of the needle, would by its expansion compensate nearly for the diminution of the magnetic moment.

In both cases such computations could only be considered as rough guides to the instrument-maker who, by a few experiments at different temperatures, might be able to attain a more accurate compensation.

Magnets.—My experience of small magnets is not very great, but I feel inclined to prefer thin parallelopipedal magnets of a length of about six inches for variation instruments in a fixed observatory, to either the large or smaller sizes. If building them of thin bars would diminish the temperature coefficient, would the compound magnet be equally permanent? For variation instruments, mirrors and scales would be preferable to collimators for such small magnets.

The whole subject of the best forms and kinds of magnets, the best for permanency, intensity, and smallness of temperature co-efficient, requires a careful investigation. I am not aware whether any considerable use has been made of the labours of the Rev. Dr Scoresby, Mr Petrie, and others, for the purposes of an observatory.

Instruments for Absolute Determinations.—These instruments require to be placed at a distance from the others, and in a place wholly free from iron.

The Inclinometer.—This should have the needle placed apart from the reading circle, and as little metal should be employed as possible for the base: if the makers would take greater pains in obtaining metal without iron, such precautions would be less necessary. Such instruments as those made by M. Repsold of Hamburgh, with reading microscopes and short needles, seem best fitted for good observations.

In order to determine the error of old instruments due to the iron in the circle or general structure, the following process might be employed with advantage: Connect the needle with a beam carrying a lens and scale, or a mirror, with the scale at a distance,—suspend this by a silk thread so that the axle of the needle may occupy its usual position in the circle, the latter with the whole apparatus being placed horizontal and the agate planes being removed, adjust a telescope so as to read the scale, and turn the circle, &c., round, so that a given division shall occupy different azimuths; then from the variations of scale reading (the changes of declination being subducted), the effect of the circle, &c., upon the needle in different positions is determined; and from these the effect upon the needle, when the whole apparatus is vertical, may be easily obtained. This process will be found much more satisfactory then than that by vibrations. I would only farther suggest, that the process should be repeated, with the poles changed, and that the position of the needle should be obtained for the apparatus away; since in some needles I have found that the intensity of saturation differs with the end which is made north, and that one end is sometimes much weaker than the other.

The Horizontal Force-Measurer.—For a fixed observatory, I feel inclined to prefer thin parallelopipedal magnets, perhaps about 8 inches long, with deflections of from 3° to 8° according to M. Gauss's method, rather than the small magnets and large deflections by Dr Lamont's. If, however, the 4-inch magnets are used I prefer a modification of Dr Lamont's method which Dr Lloyd has employed, in which the deflecting bar is kept always at right angles, not to the suspended magnet, but to the magnetic meridian. This subject requires considerable examination; the discordant results obtained from different instruments, and from the same instrument with different magnets, are too considerable not to require explanation. The deflecting magnet should never be touched with the hand during observations, but it should be lifted by a carriage or chair. The expansion of the distance beam should not be omitted in the computation of the absolute intensity; where it is of brass the effect of this omission may be quite marked in comparing observations during summer and during winter. I may touch upon the whole subject again in another way.

Having noticed those points which have occurred to me as of principal importance, or as more or less new, I cannot conclude this report without adverting to the assistance which I have received from different gentlemen during my labours at Makerstoun.

To yourself I have been especially indebted. The foundation and support of the observatory is a thing, of itself, which might be noticed as a matter for gratulation. It is rare for persons with much more extensive means to have done so much for science,—while they were themselves living and capable of otherwise employing their wealth—it is rare for them to have done so much even by their bequests. This reason for thanks I possess only in common with other lovers of science. It is for your personal kindness, the friendship with which you have honoured me, the suggestions and aids which I have derived from you in every matter likely to advance the cause that you have had so much at heart, and which were likely to enable me to perform those duties devolving upon me with satisfaction and comfort to myself,—it is for these, and many similar kindnesses, that I have to offer you my most grateful and most hearty thanks.

You have acknowledged, in the first part of the Makerstoun Observations, the assistance which you derived from the advice of Professor J. D. Forbes as to the formation and continuance of the Makerstoun Observatory, and I have to acknowledge my obligations to him in a more extensive way. As his pupil I am indebted to him for his valuable instructions, and especially for that love of strict science which he has sought with so much success to diffuse among his students in the University of our Scottish metropolis, whether by his prelections or by his example, through those original and laborious scientific investigations which he has prosecuted so successfully. In some of the latter of these I had the pleasure and good fortune to assist him, benefiting as I hope I did, by that cautious and careful spirit which distinguishes his researches. Besides many other kindnesses, I owe to his recommendation your selection of me for the care of your observatory: since then, I have been in constant communication with him, either as my friend and adviser, or in his capacity as secretary of the Royal Society of Edinburgh, in whose Transactions the work of your observatory has appeared.

To the Rev. Dr H. Lloyd, the excellent President of the Royal Irish Academy, I also owe my best acknowledgments. It was to him that the observatory owed its first scheme of observation, and much attention and examination of the earliest observations. I have also been indebted to him in frequent communications, and, above all, to his published papers on the instruments and processes of observatory work, without the use of which my own progress, and that of most of the Directors of our Colonial Observatories, would have been difficult and painful labour.

Mr Airy, the Astronomer Royal, I have before thanked for the instruction which I received in the Greenwich Observatory before undertaking the charge of yours; but I have also to thank him for the readiness and willingness with which he has invariably thrown open to me any matter connected with the observations at Greenwich, which I may have desired for comparison with our own.

I have already noticed the valuable grant of the Royal Society of Edinburgh towards the printing of the volumes of Observations. I may also notice that copies of the volumes of Makerstoun Observations have been forwarded by the Royal Society to the institutions and individuals, given in the Addendum to this Report, in addition to those entitled to receive them as portions of the Edinburgh Transactions.

Permit me to conclude by expressing my hope that these labours, to which

I have devoted my best energies, may ultimately be found to have added something of importance to our knowledge of the wondrous works of that Infinite Being, the Creator of all things and the Redeemer of men.

And I have the honour to be,

Sir,

Your very obedient,

And very humble servant,

JOHN ALLAN BROUN.

Edinburgh, *July* 1850.

ADDENDUM.

Besides those Institutions and Individuals entitled to receive the Edinburgh Transactions, the following have had the Makerstoun Observations forwarded to them:—

ENGLAND.

The Royal Observatory, *Greenwich.*
The Cambridge Observatory.
The Oxford Observatory.
The Durham Observatory.
The Royal Artillery Library, *Woolwich.*
The Trigonometrical Survey Office, *London.*
Colonel Sabine, *Woolwich.*
Captain Riddell.
Very Rev. Dr Peacock, *Ely.*
Rev. Dr Whewell, *Cambridge.*
S. H. Christie, Esq., *Woolwich.*
W. S. Harris, Esq., *Plymouth.*

SCOTLAND.

The Royal Observatory, *Edinburgh.*
The Glasgow Observatory.

IRELAND.

The Dublin Observatory.
The Armagh Observatory.
Rev. Dr Lloyd, *Dublin.*
The Earl of Rosse, *Parsonstown.*
E. J. Cooper, Esq., *Markree.*

COLONIES, ETC.

St Helena, Magnetic Observatory.
Cape of Good Hope, Observatory.
Cape of Good Hope, Magnetic Observatory.
Madras, Observatory.
Bombay, Magnetic Observatory.
Trevandrum, Magnetic Observatory.
Paramatta, N. S. W., Observatory.
Toronto, Canada, Magnetic Observatory.

CONTINENT OF EUROPE.

Berlin, M. Dove.
Berlin, M. Erman.
Bonn, Observatory.
Breslau, Observatory.
Cadiz, Observatory.
Dorpat, M. Kämtz.
Geneva, Observatory.
Helsingfors, Magnetic Observatory.
Kasan, Observatory.
Lausanne, M. Wartmann.
Milan, Observatory.
Naples, Observatory.
Paris, Observatory.
Prague, Observatory.
Pulkowa, Observatory.
Rome, Observatory Collegio Romano.
Senftenberg (Bohemia), Observatory.
St Petersburg, Corps des Mines.
Vienna, Observatory.

UNITED STATES.

Boston, Harford Magnetic Observatory.
Philadelphia, Girard College Magnetic Observatory.
Washington, Observatory.